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Scientific realism, scientific practice, and science communication: An empirical investigation of academics and science communicators★

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ABSTRACT

We argue that the societal consequences of the scientific realism debate, in the context of science-to-public communication are often overlooked and careful theorizing about it needs further empirical groundwork. As such, we conducted a survey experiment with 130 academics (from physics, chemistry, and biology) and 137 science communicators. We provided them with an 11-item questionnaire probing their views of scientific realism and related concepts. Contra theoretical expectations, we find that (a) science communicators are generally more inclined towards scientific antirealism when compared to scientists in the same academic fields, though both groups show an inclination towards realism and (b) academics who engage in more theoretical work are not less (or more) realist than experimentalists. Lastly, (c), we fail to find differences with respect to selective realism but find that science communicators are significantly less epistemically voluntarist compared to their academic counterparts. Overall, our results provide first empirical evidence on the views of scientists and science communicators on scientific realism, with some results running contra to the theoretical expectations, opening up new empirical and theoretical research directions.

1. Introduction

1.1. Overview and motivation

At its core, scientific realism is the view that a world exists independently of our minds and language, and scientific inquiry provides us with knowledge or (approximate) truths about both its observable and unobservable aspects. The present study aims at providing some empirical basis for connecting the scientific realism debate with the practical debate on science to public communication. In Section 1, we introduce the basics of the scientific realism debate and its societal implications. We then connect this to current practices of science communication. Afterwards we build and motivate our hypotheses (section 2), present our method (section 3), and report and discuss our results (section 4-6).

Our primary intent is to lay the groundwork for empirical research that explores the nexus between science communication and scientific realism. Our study is conceived as an initial step in this direction, and in many ways, it functions as the first piece of a larger puzzle that future research will have to build upon.

Understanding the attitudes concerning scientific realism of science communicators is crucial, as it sets the stage for such subsequent research. Once we have a clear grasp of these attitudes, we can then delve deeper into examining how they influence their communication practices. Our foundational empirical work consists of assessing the realist viewpoints of both scientists and science communicators and drawing comparisons between them. Additionally, we juxtapose scientists involved in theoretical work with those in applied domains. Our findings suggest that.

(a) While both groups lean towards realism, science communicators tend to be more antirealist compared to scientists within the same academic disciplines.
(b) Theoretical academics do not exhibit a greater or lesser realist attitudes than those doing applied work.
(c) No significant differences were observed in terms of selective realism. However, science communicators were markedly lower on epistemic voluntarism compared to their peers in academia.

The scholarly significance of this work is to function as an empirical underpinning for the philosophical exploration of realism and antirealism in the context of science communication. Our argument centers on the observation that reporting within the hard sciences frequently employs naïve realist terms. This tendency could potentially

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hinder effective knowledge dissemination to the public and may have unwanted public funding outcomes.

Our secondary intent is to provide a partial replication and extension of Beebe and Dellsén's (2020) study on scientific realist attitudes of scientists. We thus connect our work also to their motivation which is as follows:

Despite extensive discussions on various strands of scientific realism, there has been a noticeable lack of empirical investigations into the perspectives of actual working scientists on these topics. This is particularly significant given that these scientists can provide insights on these debates, both epistemically and experientially, especially if one has a naturalist outlook. They have a deep understanding of the epistemic merits of the theories they engage with, given their access to a broader spectrum of evidence compared to other stakeholders, such as philosophers of science. Moreover, these scientists are uniquely positioned to elucidate the nature of their acceptance of particular theories.

The central goal of Beebe and Dellsén's research and the secondary intent of our research is thus to bridge this empirical void by probing the attitudes of working scientists towards the various facets of scientific realism and, in our case, extending this to science communicators. Given the currently large discussion on replication failures in the social sciences and subsequent calls for replications, we took the opportunity and designed parts of our study as a replication of Beebe and Dellsén's and indeed replicated a variety of their findings.

1.2. Scientific realism basics

The scientific realism debate is concerned with the relation between our scientific theories of the world and the world itself. One typically differentiates axiological formulations of scientific realism, i.e., in terms of the aims of science, from those in terms of epistemic achievement. Regarding the achievement formulation, classical realists typically commit themselves to something akin to the following: Most current mature and predictive successful scientific theories are approximately true. Formulations in terms of aims are frequently associated with Popper (1983) and Van Fraassen (1980).

Popper (1983, p. 57) explains that, for the realist, "truth – absolute truth – remains our aim". Conversely, van Fraassen (1980, 12) spells out the anti-realist aim in terms of *empirical adequacy*, i.e., truth "about the observable things and events in this world" (Van Fraassen, 1980, p. 12). Contemporary accounts differentiate three dimensions of scientific realism (Psillos, 1999; Chakravartty, 2017b, section 1.2).

Metaphysical Realism	The world (and its objects) exists independently of mind and language.
Semantic Realism	Scientific claims have truth values, and such claims have to be interpreted literally.
Epistemological Realism	We have justified beliefs (or knowledge) about the world through scientific investigation. This concerns observable and unobservable aspects.

The two most prominent arguments in the realism debate are as laid out below.

(1) *For Realism: No-Miracle Argument (NMA)*

"The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle." (Putnam, 1975a, 73)

"The basic argument for the unobservable entities is simple. By supposing they exist, we can give good explanations of the behaviour and characteristics of observed entities, behaviour and characteristics which would otherwise remain completely inexplicable." (Devitt, 1996, 108)

(2) *Against Realism: Pessimistic Meta-Induction (PI)*

"For every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring." (Laudan, 1981, 35)

"The following meta-induction becomes overwhelmingly compelling: just as no term used in science of more than fifty (or whatever) years ago referred, so it will turn out that no term used now (except maybe observation terms …) refers." (Putnam, 1978, 25)

Further arguments against realism are underdetermination arguments (Chakravartty (2017b, p. 3.1; Quine, 1951, pp. 20–46) and unconceived alternatives arguments (Rowbottom, 2016; Stanford, 2006). Further arguments for realism are arguments from corroboration (Hacking, 1983) and the default argument for realism (Quine, 1955; Wylie, 1986). We will not test these specific arguments in our empirical research, and thus do not discuss them at length here.

In the more recent debate, various so-called 'selective realist' responses to the anti-realist challenges developed. Selective realists claim that rational believing is restricted to various discriminative portions of current science, such as structures only (French & Ladyman, 2011, pp. 25–42; Worrall, 1989), or entities only (Hacking, 1983), or properties only (Chakravartty, 2008), or only to various stable, invariant portions of theories, including entity claims (Kitcher, 1993; Psillos, 2021).

Another recently widely discussed topic is the question of *stances* and *epistemic voluntarism*. Going back to Carnap (1937, p. 3) and Van Fraassen (1989; 2002); they view *empiricism* not as a set of beliefs but rather something non-doxastic, non-cognitive – *a stance*. As Chakravartty (2017a, p. 47) puts it: "A stance is an orientation, a cluster of attitudes, commitments, and strategies relevant to the production of allegedly factual beliefs." Since stances are on many conceptions (see e.g., Chakravartty, 2017a; Van Fraassen, 2002) neither doxastic nor purely based on facts but rather expressions of commitments, choices, and desires, Van Fraassen (2002, pp. 81–89) argues that one ought to be liberal about stance-choice – a position he calls *epistemic voluntarism*. As Chakravartty puts it:

While all defensible stances must pass the test of rationality, there is no one answer to the question of what a responsible epistemic agent should value. Epistemological values are variable, and though an individual may change her mind about them, she need not. (Chakravartty 2018, 233)

From this he concludes that to "subtract the usual judgment that at most one party to these disputes is, in fact, correct" (Chakravartty 2018, 232). Chakravartty commits to a realist stance, van Fraassen to an anti-realist one.

1.3. Societal importance

The realism debate is best conceived as a cognitive endeavour. As such, societal impacts are usually not discussed. Two topical exceptions come to mind. First, Cartwright (1999, 18) argues that a naïve realist outlook on science with its too high trust in its truth-finding capacities can lead to misguided funding decisions. It might lead to the sacrifice of research programmes that can reliably improve lives for research programmes that aspire to an unrealistic ideal of scientific truth-acquisition. Conversely, Brown (2001) and others have argued that we should learn from the controversies initiated by the *strong program*, *social constructivism, postmodernism,* and the following *science wars*, that the question of scientific realism can have an enormous influence on politics and society, and some anti-realist proposals can lead to an offspring of pseudoscience and science denial.[1]

Further, the current literature on science communication is substantially focused on ethical considerations by themselves or on possible tensions with epistemic considerations.[2] Even though various philosophers of science have proposed guiding principles for communicating scientific findings to enhance public trust and objectivity on the background of scientific values (e.g., Elliott & Resnik, 2014), the current literature on science communication largely lacks a sustained discussion on whether science communicators should communicate scientific insights with a more realist or anti-realist narrative, or with a reflection on both.

Particularly, from the perspective of accurately representing the state-of-the-art research in philosophy of science, this is a pressing concern since the realism debate is far from settled; selective responses rise in prominence, and voluntarist responses emerge. Favouring one side on epistemic grounds might carry bias and might then add to the current problems with public misinformation about science (Posetti & Bontcheva, 2020; Kitcher, 2020, pp. 89–120).

We think that one foundation for starting to theorize about science communication in the context of scientific realism is an empirical investigation on current science communication. For this, we start with an investigation about what science communicators currently think about scientific realism, and how this differs from academics.

1.4. Current practices of science communicators

The present study compares realist and anti-realist attitudes of science communicators and scientists. We want to give a brief overview of our pre-study expectations. It can be casually observed that science communication is often done in scientific realist terms, especially when it comes to findings in 'hard' sciences, such as physics, chemistry, and biology. For a systematic evaluation of this claim, a corpus analysis would be needed. For the present paper, however, we can only provide some case-based circumstantial evidence that is apt for further empirical investigation. We stick to striking examples; specifically, the black hole pictures of 2019, the current COVID-19 pandemic, and the Higgs discoveries from 2012. To start with the black hole pictures: First communicated by the European Southern Observatory, the article was entitled: "Astronomers Capture First Image of a Black Hole" (Falcke et al., 2019a), Falcke et al., 2019b title subsequently adopted in dozens of venues (e.g., Falcke et al., 2019b; Falcke et al., 2019c). Other titles are even more direct: Science.org titles it "For the first time, you can see what a black hole looks like" (Clery, 2019). These are straightforwardly realist formulations. Even various realists might be uncomfortable in claiming that, on this picture, you can actually 'see what a black hole looks like'. For anti-realists, however, it would be completely inconsistent to claim that there is an image of a black hole, or that one can see what a black hole looks like, while simultaneously being at least agnostic towards their existence (except for semantic anti-realist communication which we will discuss later). By looking at popular venues for communicating science findings to the public, we were not able to find a single anti-realist formulation of this event.

When the public gets informed about particles, chemical bonds, or the spread of a disease, it is almost exclusively presented within a straightforwardly realist picture. E.g., anti-realist phrasings with a realist semantics of the Covid pandemic sound particularly strange to our ears: 'The observed phenomena are such, as if the SARS-CoV-2 virus existed (to use a surrealist formulation)' or 'we constructed a theory that postulates SARS-CoV-2, which might or might not exist, in order to predict observable developments, such as excess death rates, and improve survivability if a specific group of phenomena appears.'

One final example: In 2019, scientists at CERN laid out a proposal for a new Large Hadron Collider (LHC) four-times larger than the current LHC, proposing a roughly €21 billion investment (Castelvecchi, 2019, p. 410). The financing discussion is, again, done in realist terms, by referring to the success of the current *LHC*. Brumfiel writes that "if the Higgs particle did not exist, there would be less than a one-in-a-million chance of getting these data by chance" (2012, 147). This was the first article, published in *Nature*, on this scientific episode. Contrary to what the formulations by Brumfield suggest, this discovery does not prove anti-realism about Higgs particles extremely unlikely. Brumfield simply operates with the (naïve) realist presupposition that the existence of the Higgs particle is the only reasonable explanation for the observed phenomena. What would anti-realists say? Van Fraassen (1980), for instance, argues for explaining the novel success of science via a mechanism of natural selection. Stanford (2000) argues that predictive similarity can explain the success. All of these are explanations of how scientific theories can be successful without directly relying on the existence of the proposed entity and without leaving it up to chance that they are successful. Whether we find those anti-realist explanations of the success of science convincing or not, factually, science communicators pick the realist side in almost all cases.

Another interpretation might be that such science reporters are fictionalists or general semantic anti-realists who adopt a realist language. We will revisit that shortly, but first, let us address anti-realist reporting that reflects an anti-realist linguistic surface structure. Concerning the Higgs boson episode, such anti-realists cannot say that the greatest success of the current LHC is "discovering the Higgs boson", as Brumfiel (2012, p. 147) writes in *Nature.* Anti-realists saying this would bend the meaning of the word 'discovery' beyond recognition. You cannot discover something that is not there, and you cannot say that Higgs boson was discovered if you are actually agnostic about whether there is such a thing as a Higgs boson.[3]

Instead, anti-realists who use clear and transparent language need a different formulation. They can contend that the greatest success of the *LHC* is discovering.

(i) that the experimentally produced phenomena are as if the Higgs boson exists,
(ii) that the experimentally produced phenomena are consistent with our current standard model of particle physics, especially concerning the Higgs mechanism,
(iii) that the current standard model of particle physics, especially concerning the Higgs mechanism, is empirically adequate,
(iv) that the standard model, especially concerning the Higgs mechanism, predicted the novel observations of the *LHC* correctly.

These are all fair formulations of various anti-realist perspectives, even though not all of them work as intended by anti-realists, as (i) – (iii)

[1] Also see De Vrieze (2017), Hamilton (2017), Almassi (2019), and Whooley (2018, 252).

[2] For instance, it is discussed whether some scientific knowledge should not be communicated (Kourany, 2016), might reinforce unwanted cognitive and behavioral outcomes (Peters, 2024), can be distributed more fairly (Medvecky, 2018), can be communicated to not harm politically marginalized groups (Saul, 2018) and reflects on the epistemic and moral problems of experts testifying outside their domain of expertise (Gerken, 2018).

[3] This goes back to Ryle's (1953) observation about the meaning of what he calls 'words with success grammar'. You can believe to observe a particle without the particle being there, but you cannot observe a particle that is not there.

underplay the surprising success.[4] In any case, what the anti-realist cannot say is that in the experiments of the *LHC*, the Higgs boson was *discovered*. An exception to this is the aforementioned fictionalist and semantic anti-realist perspectives. While they employ a realist linguistic surface structure, their ontological stance, upon interpretation, explicitly excludes entities such as the Higgs boson. From the perspective of science communication, this raises the pivotal question: What combination of positions would produce the form of realist science communication that we see? If science communicators reflect their attitudes in their communication, and given circumstantial evidence for prevailing realist communicative styles, theoretically consistent science communicators would need to predominantly adhere to one of the following configurations (given the three dimensions of scientific realism mentioned in section 1.2).

(i) They endorse a metaphysical, epistemological, and semantic realist stance.
(ii) They adopt a metaphysical or epistemological anti-realist stance (or both), but pair it with a semantic anti-realist stance that nonetheless employs realist surface language.

Both (i) and (ii) yield realist surface language. Any other combination would produce anti-realist surface language.

1.5. Science to public communication norms

Following Cartwright's argument that a too naïve realist outlook will lead to wrong funding decisions (see section 1.2), then the LHC case is better communicated in nuanced terms, since it will facilitate a public with expectations that reflect the real epistemic limitations and potentials better. The same holds for communication to the relevant funding bodies even though the picture gets more complex since there usually are several layers of communication involved, such as expert-to-expert, expert-to-wider-scientific-community, and expert-to-public. Given that the state of the current research on the realism debate is not decisively favouring one side, communicating only the realist side to the relevant funding body must lead to an overfunding of highly theoretical research where the expected payoff is mostly cognitive, as the expectations of what such research can provide is biased towards the realist side. There might be an argument to be made that the COVID-19 episode is better communicated in realist terms in order to combat science denial and communicate practical decision making effectively.

Any such normative arguments are still in their infancy, but before philosophers can do more normative work on how science communicators should communicate their findings in contexts such as these, it will be very helpful to do some empirical work first. As such, our primary intent is to lay the groundwork for empirical research that explores the nexus between science communication and scientific realism.

2. Hypotheses

We investigate three hypotheses, pre-registered on the Open Science Framework alongside analysis plans.[5]

Null Hypothesis 1. There is no difference in scientific realist views between academics (in physics, chemistry, and biology) and science communicators (in physics, chemistry, and biology) in their respective fields.

It is difficult to give a prediction for Null Hypothesis 1. To do so would necessitate evidence delineating the relationship between science communicators and academics in terms of their realist attitudes, and currently, there is no relevant data on this. As laid out in *section 1*, there is some case-based evidence that science communication in physics, chemistry, and biology is frequently done in realist terms and this might be explained by the attitudes of said communicators. As we know from Beebe/Dellsén (2020), academics in physics, chemistry, and biology exhibit strong realist attitudes but some exhibit anti-realist attitudes as well. Taking this into account, our directional prediction for Null Hypothesis 1 posits that science communicators exhibit stronger realist tendencies than academics in their respective fields. However, this prediction, as previously mentioned, remains highly speculative given the current scope of research.

Null Hypothesis 2. There is no difference within academic disciplines in scientific realist views between those who work in theoretical subfields and those who work in applied subfields.

Our directional prediction for Null Hypothesis 2 is that we expect those academics working in more applied contexts to be more realist. There is no direct evidence for this expectation but there is some theorizing that this holds for experimentalists which might have some transfer to applied scientists. For instance, Hacking claims that "[t]he vast majority of experimental physicists are realists about some theoretical entities, namely the ones they use. I claim that they cannot help being so" (1983, 262). He reasons that by causally manipulating entities in the laboratory, the experimentalist cannot help but think that those entities are really there. As Hacking says about electrons, "[i]f you can spray them, then they are real" (1983, 22). This gives some rationale for thinking that academics working in more applied contexts are more realist and thus informs our theoretical prediction even though the populations of experimentalists and those working in applied contexts do not completely overlap.

Null Hypothesis 2 is integrated to address an important internal dynamic within the realm of academia itself: to understand if the nature of one's work, theoretical versus applied, dictates their leanings towards realism. By doing so, we believe we can set a more robust backdrop against which the findings of Null Hypotheses 1 and 3 can be contextualized.

Null Hypothesis 3. There is no difference in selective realism and voluntarism between academics (in physics, chemistry, and biology) and science communicators (in physics, chemistry, and biology) in their respective fields.

Our directional predictions for Null Hypothesis 3 are that science communicators are more selectively realist and less voluntarist. We do not have a strong rationale for this as there is no empirical data available. However, one would expect science communicators to be more selectively realist than scientists since their reflections on science on a broader scale might make for a more nuanced view even though one could also argue that because science communicators do not know the nuances of science as well as scientists, they would be less inclined toward selective realism Furthermore, communicators might be less voluntarist given that science communication is usually done in straight realist terms, and one would expect voluntarists to communicate in a more nuanced way.

One central difference to previous work by Beebe and Dellsén (2020) is that we are not directly interested in differences between academic disciplines, such as between chemists and biologists. Rather, we are interested in differences between those that do academic research (in a given discipline) and those that engage in science communication (in a

[4] It is frequently contended that surrealist formulations, such as (i) are merely another way of expressing empirical adequacy, i.e. (iii) (cf. Musgrave, 2007). Scientists frequently speak in a way of (ii), often to avoid the word 'truth'. In reality, 'consistency' is much too weak of a term, even for anti-realists, since a broad range of theoretical models will be consistent with this discovery – it is also consistent with me licking ice cream three days ago. Consistency definitely does not capture what many scientists think it does (something observed by Stove (1982, p. 15)). Version (iii) is better than (ii) in this regard. However, only (iv) really addresses what is so special about the success of the experiment, while not invoking realist terms.

[5] https://osf.io/63xe8/?view_only=600f132be526442aa46902f88499da61.

given discipline).

3. Methods

3.1. Participants

We collected our samples from two populations: scientists and science communicators. For our scientist sample, we collected publicly available email addresses from faculty and doctoral students in physics, chemistry, and biology departments from 24 universities in the United States. In total, we sent our survey to 8483 email addresses, and failed to deliver them to 456 of those. For our science communicator sample, we sent emails to two mailing lists (Public Engagement with Science (PSCI) and Public Communication of Science and Technology (PCST)) as well as a science communication focused online newsletter (The SciCommer Newsletter). Here we estimate roughly 9000 email addresses. This study has received ethics approval.[6]

We collected our data between 15th and 30th of June 2022. We stopped data collection 24 h after no new complete responses were received. Our estimated response rate for the scientist sample was 1.82%, while the estimated response rate for the science communicator sample was 1.60%. In part, this low response rate may be explained by the relatively high number of emails that could not be delivered (over 5% in the academic sample) or ended in spam filters (which we cannot estimate numerically). Because we do not have a clear idea of how many emails bounced for the science communicator sample, we calculate our response rate regarding emails sent, though this makes it a lower bound estimate. Further, low response rates are relatively common in expert samples.[7]

3.2. Procedures and measures

All participants, i.e., scientists and science-communicators, were presented with an almost identical questionnaire consisting of 11 central items. The only difference on top of the main questionnaire was the inclusion of an additional item for scientists. This item aimed at measuring how applied or theoretical any individual scientist's work was. At the beginning of the survey, participants were informed about the content and the expected duration of the survey. We then obtained informed consent by having participants explicitly consent to participation.

We collected three sets of variables for our main outcome variables: the Full Scientific Realism Scale, attitudes towards selective realism, and attitudes towards voluntarism. Items 5, 6, and 8 were reverse items, thus being re-reversed prior to all analysis.[8] The Aggregate Score is a composite score of the first nine items relating to different facets of realism. For a list of all items that make up the Aggregate Score (items 1 through 9) as well as our selective realism (item 10) and voluntarism (item 11) items, see Table 1.

In our questionnaire, the *items 1–6* and *9* test various aspects of the three dimensions of scientific realism, as explained in *section 1.1*, and *items 7* and *8* relate to the two main arguments – the *no miracle argument* for realism and the *pessimistic induction* against realism. Furthermore, one core question of the scientific realism debate is how progress in science occurs and whether it can be conceptualized in terms of truth. This concerns *item 4*.

Table 1
Full set of Items.

1. Metaphysical Realism (MR1)	The objects and phenomena studied by science exist independently of how we conceive of or think about them.
2. Metaphysical Realism (MR2)	Even if we do not know which scientific theories are ultimately true, there is an ultimate truth out there.
3. Epistemic Realism (ER)	Our most successful and rigorously tested scientific theories are at least approximately true.
4. Progressive Realism (PR)	Progress in science is a matter of getting closer and closer to the underlying truth about reality.
5. van Fraassen Anti-Realism (vFr1) - R	In order to go about their daily business as scientists, scientists do not need to believe that any of the theories they rely upon provides them with literally correct descriptions of the world.
6. van Fraassen Anti-Realism (vFr2) - R	Ultimately, the aim of a theoretical science is to systematize observed phenomena and predict new phenomena. Correctly explaining phenomena by postulating underlying objects and mechanisms is at best merely instrumental to the ultimate aim.
7. No-Miracles Argument (NMA)	The best explanation for the remarkable success of our best scientific theories is that they accurately depict an underlying reality.
8. Pessimistic Induction (PI) - R	It is a fact that the history of science contains many predictively successful theories that turned out to be fundamentally[a] mistaken. Thus, inferring from the success of our current best scientific theories that they are fundamentally right is mistaken.
9. Semantic Realism (SemR)	Scientific theories frequently postulate the existence of various objects and mechanisms. Those theories are only true if those objects and mechanisms actually exist as described by the theory.
10. Selective Realism (SelR)	We can differentiate between better and worse supported elements of our current best scientific theories. There are good reasons to think that only the better supported elements depict an underlying reality.
11. Voluntarism (Vol)	It is reasonable to judge that our most successful and rigorously tested scientific theories are at least approximately true, but it is also reasonable to refrain from such a judgment.

Notes: Full set of items including their abbreviations.
[a] Concerning the phrasing "fundamentally mistaken" which also appears in Beebe/Dellsén's (2020) PI. Defenders of PI usually do not want to say that there was quite something mistaken about the caloric theory of heat, they want to say that there were multiple things *fundamentally* wrong with it (e.g., especially that caloric simply does not exist) even though it was a successful theory. This motivates that we adopt this word choice.

Various of these items were adapted from Beebe and Dellsén (2020), some have been shortened and significantly reworked, others have been added. We intended to keep the scale as close as possible to the one developed by Beebe and Dellsén (2020) in order for our study to additionally function as a partial replication (though our study aims diverge significantly). As such, our core *items 1, 3, 4, 5*, and *7* are identical to the one's by Beebe and Dellsén. We changed *item 2* only slightly by focusing on 'knowledge' and not on 'certainty', as we think that the word 'certainty' might unnecessarily put scientists off. We also rephrased the second van Fraassen item (*item 6*), now expressing it in terms of aims. Thus, we stick more closely to van Fraassen's formulation, though the meaning is equivalent to Beebe and Dellsén's. However, it may be that leaving out aim talk contributed to the surprising outcome of their study that van Fraassen's characterization of scientific realism failed to cluster with more standard characterizations of anti-realism and also did not cluster with the second van Fraassen item. It is thus independently interesting whether Beebe and Dellsén's results replicate with a slightly different formulation. *Item 6* functions as testing aim versions of scientific realism.

One potential concern regarding item 5 is that the empiricist alternative—that scientists should believe those theories to be empirically adequate—may not be immediately evident to respondents. Accepting item 5 could potentially be misinterpreted as an extreme skeptical

[6] Ethics approval code and institution have been anonymised to fully enable double-blind peer review.
[7] We address the low response rate in our limitations sections.
[8] Note that we did not indicate the reversing and re-reversing in detail in the pre-registration. However, since these items are undoubtably anti-realist items, it is clearly justified to deviate from the pre-registration on this detail. Reporting the results exactly as pre-registered in this regard would not make sense theoretically, explaining our deviation from pre-registered protocol.

viewpoint about the whole scientific enterprise. Nonetheless, we anticipate that seasoned scientists will understand the function of the term 'literally' and since we already modified Beebe and Dellsén's (2020) second van Fraassen item we at least wanted to keep this one originally, as to preserve the replication character of our study.

Beebe and Dellsén (2020) added items for convergence, theory-ladenness, scientism, and disagreement (their *items 9–14*; see Beebe/Dellsén 2020, 340). We agree that these are important facets in the realism debate but think that they are slightly more on the periphery. As such, we did not test them as to keep the questionnaire as short as possible to increase survey completion rates. However, we added an item on semantic realism (*item 9*) to test the third dimension of scientific realism. Furthermore, we added one item for selective realism and epistemic voluntarism respectively (*items 10* and *11*) to check these two widely discussed positions.

Participants were asked to rate their agreement or disagreement with these items on a 5-point Likert scale ranging from '1 – Strongly Disagree' to '5 – Strongly Agree'. We anchored the poles as well as the mid-point numerically to emphasise equidistance between points as we used the data as interval data.

We collected several additional variables from all participants. First, to double check our categorisation of academics into physicists, chemists, and biologists, we asked academics to self-categorise their academic work into one of the three disciplines (or select 'Other' or 'None of the above'). Science communicators also received this question, though as our data collection method did not allow for a categorisation on our end, these answers were used as the sole criterion for disciplinary categorisation. Note also that for all samples, several fields could be selected to account for inter- and multidisciplinary work.

Second, we asked scientists to rate on a scale from −3 ('Highly theoretical') to +3 ('Highly applied') how they viewed their own work. Science communicators did not receive this item. Third, we collected several additional items that we used as control variables: age, gender, having a PhD, and having ever taken a philosophy class. Both scientists and science communicators were shown the same set of demographic survey questions at the end, and all analyses control for this set of demographic variables. All participants could enter a lottery for a $100 Amazon gift card as a participation incentive.

To reduce participant survey fatigue, we randomly selected six items to be presented together on one page and the remaining five on another page. Additionally, we added an attention check that asked participants to click 'Disagree' and excluded participants from all analyses who did not do so. See Fig. 1 for an overview of the experimental procedure.

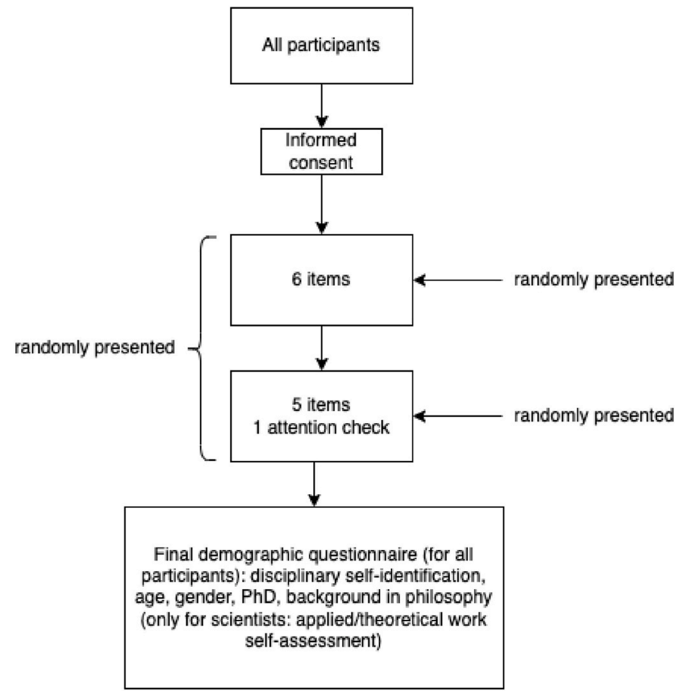

Fig. 1. Experimental procedure overview.

4. Results[9]

Below (see Table 2) we report the demographics and descriptive statistics for both our science communicator and our scientist samples after all our exclusion criteria are applied (specifically, 23 participants failed the attention check and one academic indicated that none of the disciplines presented to them matched their area of work). As pre-registered, here, and in all following analyses, our scientist sample (n = 130) is categorised by our categorisation based on their institutional affiliations. Our science communicator sample (n = 137) is categorised by self-identifying as covering any of the academic fields. As our recruitment approach was less targeted by design for our science communicator sample, some participants did not see their work as covering any of the three disciplines that we studied, resulting in the sum of all responses to be lower than the total sample.

One central variable for this paper is the sum score of the 9-item scientific realism scale, which we call the *Full Aggregate Score*. We pre-registered to not include *selective realism* and *voluntarism* in our main score as they constitute theoretically distinct concepts.[10] In Table 3 below, we report the mean, standard deviation, as well as 95% confidence intervals for all 11 items after reversing the anti-realist items.

Table 2
Descriptives of the full sample.

	Age	Gender (female)	PhD	Philosophy Background
Scientists (n = 130)	M = 40.2 (19.6)	30.3%	50.8%	55.4%
Physicists (n = 56)			48.2%	58.9%
Chemists (n = 37)			43.2%	54.1%
Biologists (n = 37)			62.2%	51.4%
Science Communicators (n = 137)	M = 43.1 (13.3)	54.0%	49.6%	55.1%
Focus on Physics (n = 48)			52.1%	47.9%
Focus on Chemistry (n = 28)			50.0%	50.0%
Focus on Biology (n = 51)			41.2%	62.7%

Notes: Full sample with academic field affiliations for the scientist and the science communicator sample, including age, gender, doctorate, and philosophy background.

For a density plots of each individual item, again after reversal of anti-realist items, see Fig. 2.

Additionally, in Fig. 3, we outline the full correlation matrix for the full sample and all eleven items, reporting Pearson correlation coefficients. This includes all items after reversal of reverse-coded items, with higher scores indicating agreement with realism.

We also report a pre-registered exploratory factor analysis of the nine items making up the Full Aggregate Score to extract the main scientific

[9] All analyses are conducted in the R-based GUI 'Jamovi' (The Jamovi Project 2021; R Core Team 2021) and all code books are available in the submission materials and will be uploaded to the Open Science Framework after acceptance.

[10] Note that we also find this empirically, i.e., when conducting an EFA with all 11 items, these two do not load onto the main factor.

Table 3
Descriptives for the Full Aggregate Score as well as Selective Realism and Voluntarism.

	MR 1	MR 2	ER	PR	vFr 1	vFr 2	NMA	PI	SemR	SelR	Vol
M (SD)	3.99 (1.15)	4.06 (0.81)	3.46 (1.14)	3.82 (0.95)	2.80 (1.11)	2.38 (1.06)	3.66 (1.19)	2.03 (1.01)	3.37 (1.07)	3.36 (1.09)	3.50 (0.88)
Lower CI	3.85	3.97	3.32	3.70	2.67	2.25	3.52	1.91	3.25	3.23	3.40
Upper CI	4.12	4.16	3.60	3.93	2.93	2.51	3.80	2.15	3.50	3.49	3.61

Notes: Mean, standard deviation, as well as 95% confidence intervals for all items of the Full Aggregate Score as well as selective realism and voluntarism items in the full sample of participants.

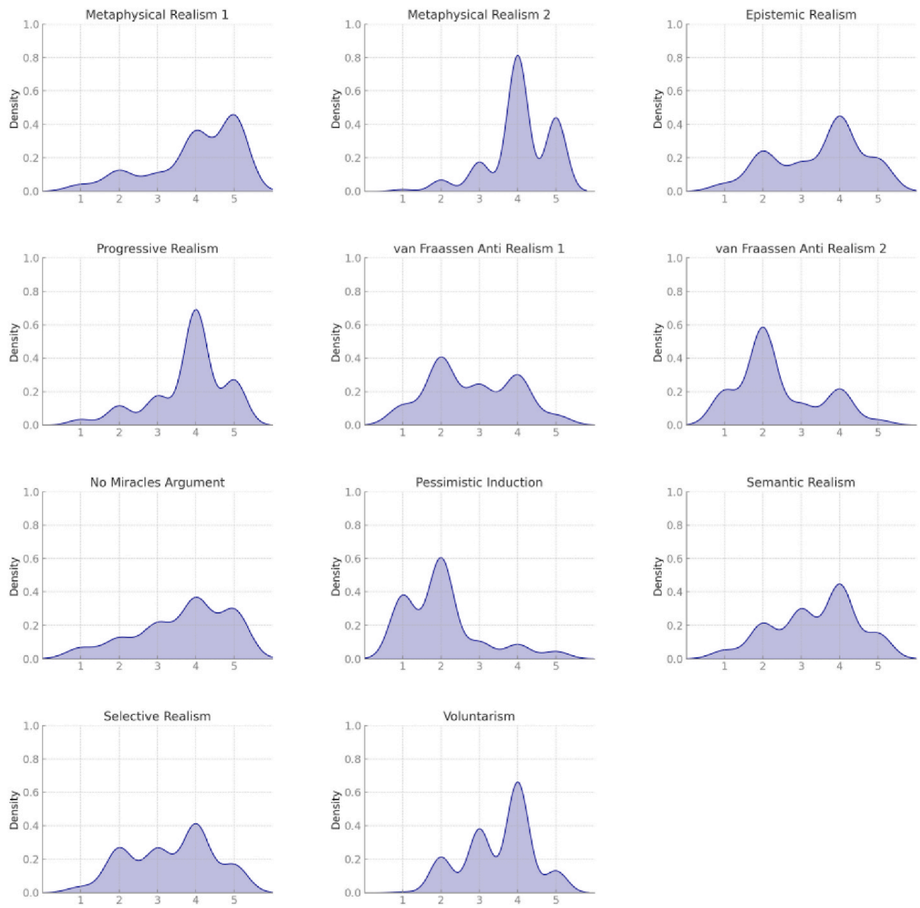

Fig. 2. Descriptive Plots for all 11 items for the Full Sample.

realism factor. We ran an EFA with the minimum residual extraction method with an oblimin rotation, suppressing all factor loadings of less than 0.40. The Barlett's Test of Sphericity was highly significant, $\chi^2(36) = 297$, $p < .0001$, and the items also showed moderate sampling adequacy based on KMO's test, having an overall MSA of 0.736, ranging from the lowest (ER) at 0.385 to the highest (MR 1) at 0.820. In Table 4 below, we report the factor loadings as well as the uniqueness of each individual item.

These results indicate the extraction of two factors. However, because the second factor only has a single item, we report all analyses with the first factor only which constitutes our main measure of scientific realism. We call this factor 'Scientific Realism Scale - Factor 1' (SRF1), consisting of the Metaphysical Realism 1, Metaphysical Realism 2, Progressive Realism, No-Miracles Argument, and Pessimistic Induction items. After reversing the reverse code items, the Pessimistic Induction item loaded negatively on our factor, leading us to subtract, rather than add it to the scale as is the standard procedure for negatively loaded items. Factor 2 includes only van Fraassen Antirealism 2 after reversal, while van Fraassen Antirealism 1, Semantic Realism, and Epistemic Realism do not load highly on either factor or form their own. For a scree plot of this EFA, see Fig. 4.

We find that both science communicators and scientists were, on average, inclined towards scientific realism. We compared the mean responses for the SRF1 against the middle point of the scale. We find that science communicators, $t(136) = 6.84$, $p < .001$, $d = 0.0.585$, as well as scientists, $t(129) = 14.4$, $p < .001$, $d = 1.27$ differ significantly from the neutral middle point. We also find that both science communicators, $t(136) = -6.19$, $p < .001$, $d = -0.529$ and scientists, $t(129) = -7.30$, $p < .001$, $d = -0.641$ are significantly more antirealist on SRF2. This is replicated with respect to van Fraassen anti-realism item 1 for science communicators, $t(136) = -2.26$, $p = .025$, $d = -0.193$ but not for scientists, $t(129) = -1.89$, $p = .061$, $d = -0.165$.

For Null Hypothesis 1, we tested whether scientists and science communicators in their respective fields were comparably more scientifically realist. We use the SRF1 as the dependent variable. We then split our sample into three pairs (and one aggregate pair), each consisting of scientists and corresponding science communicators; for example, all physicists and all science communicators that covered physics. As our

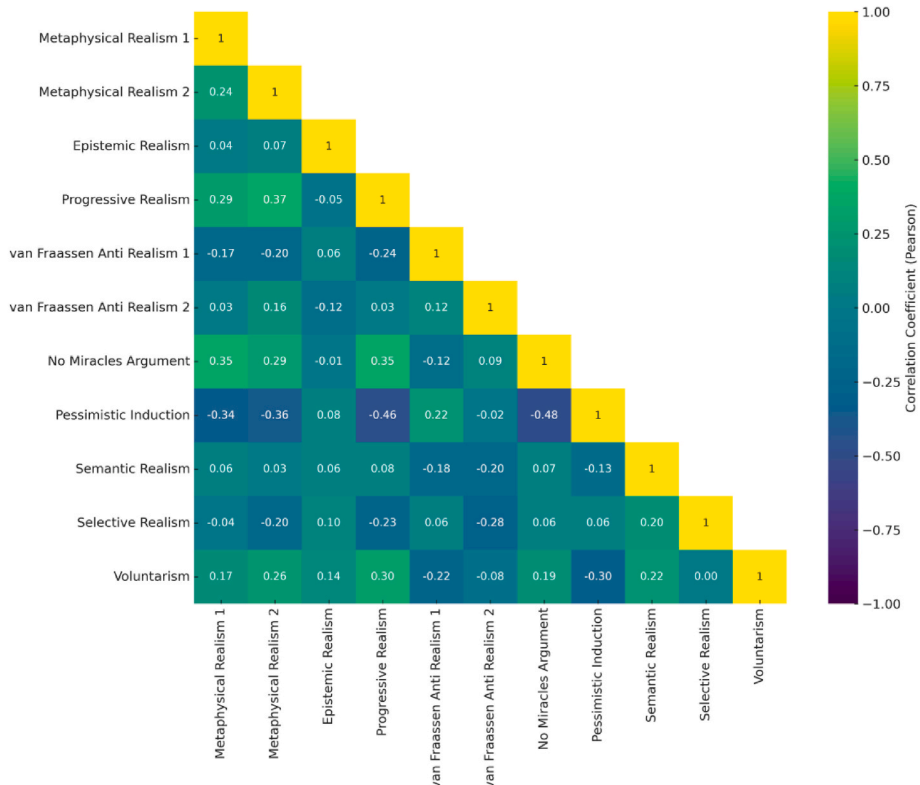

Fig. 3. Correlation Matrix for all 11 items for the Full Sample.

Table 4
Exploratory factor analysis of the full scientific realism scale.

	Factor 1	Factor 2	Uniqueness
Metaphysical Realism (MR1)	0.450		0.796
Metaphysical Realism (MR2)	0.495		0.700
Epistemic Realism (ER)			0.987
Progressive Realism (PR)	0.617		0.620
van Fraassen Anti-Realism (vFr1)			0.885
van Fraassen Anti-Realism (vFr2)		0.760	0.419
No-Miracles Argument (NMA)	0.572		0.657
Pessimistic Induction (PI)	−0.745		0.455
Semantic Realism (SemR)			0.910

Notes: Exploratory factor analysis with factor loadings and uniqueness. All factor loadings below 0.40 are suppressed.

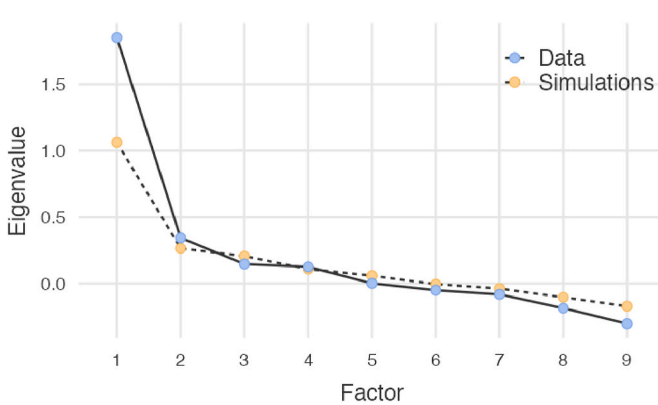

Fig. 4. Scree plot for EFA

independent variable of interest, we have a dummy for being a science communicator, where '0 = academic', and '1 = science communicator'. For our control variables, we enter dummies for holding a PhD and having taken a philosophy class, as well as our demographic controls of gender and age. This set of control variables is used for all further regressions. We report a set of OLS models below with the pre-registered alpha level of 0.0167, adjusted via the Bonferroni method, where the significance threshold of interest is indicated with '***'. We report one model for each discipline as well as one for the aggregate sample, see Table 5.

We find that for physicists, biologists, and for the aggregate sample, being a science communicator predicts a lower scientific realism score compared to academics. This effect is contrary to the theoretical predictions. All these coefficients are significant at the adjusted significance level of 0.0167, with being a science communicator working in physics being associated with a 1.8-point lower score on the SRF1 compared to their academic counterparts. Both directionally and in magnitude, roughly the same effect is found for biologists and the aggregate sample. As such, the data allow for a clear rejection of our null hypothesis, as science communicators are significantly less realist than their academic counterparts. Additionally, we find that with respect to SRF1 and the aggregate score, holding a PhD degree and having taken at least one philosophy class was not associated with higher or lower scores. However, there was some heterogeneity between the sciences, with physicists who have taken at least one philosophy class showing lower scores. Because there are no effects at the aggregate level in this analysis and further, we do not report control variables' coefficients going further. Below, we present KDE curves for a graphical illustration of these group differences for the Full Scientific Realism Score, see Fig. 5.

Further, we report a robustness check that was pre-registered as a potential additional analysis. For this check, we only analyse science communicators who did not self-report to also being scientists to ensure that our pre-registered distinction's results also hold under more stringent conditions. This drops the science communicator sample from 137

Table 5
Regression predicting difference between academics and science communicators regarding scientific realism.

	Physics	Chemistry	Biology	Full Sample
Scientific Realism Scale (SRF1)				
Science Communicator	−1.801[c] (0.711)	−0.691 (0.718)	−2.289[c] (0.881)	−1.663[d] (0.438)
PhD Degree	0.739 (0.781)	−0.537 (0.906)	0.486 (0.997)	0.250 (0.494)
Philosophy Background	−1.417[b] (0.658)	−1.414 (0.709)	−0.308 (0.872)	−0.463 (0.428)
Adj. R^2	0.060	0.010	0.069	0.051
n	104	65	88	267
Full Aggregate Score				
Science Communicator	−1.242[a] (0.730)	−1.398[a] (0.749)	−1.493[b] (0.726)	−1.219[c] (0.398)
PhD Degree	0.617 (0.802)	−0.438 (0.946)	0.161 (0.821)	0.385 (0.448)
Philosophy Background	−1.571[b] (0.676)	−0.544 (0.740)	0.435 (0.718)	−0.484 (0.388)
Adj. R^2	0.070	−0.012	0.086	0.068
n	104	65	88	267

Notes: OLS regressions predicting scores on the Full Scientific Realism Scale as well as the first extracted factor. Coefficients and standard errors in parentheses.
[a] $p < .1$.
[b] $p < .05$.
[c] $p < .0167$.
[d] $p < .001$.

to 78, where now almost two-third do not hold a PhD. Running our main analyses with this reduced sample, we find the same result, with the coefficient for SRF1 being β=-1.557 (SE = 0.530), p = .004 and for the full aggregate score at β=-1.445 (SE = 0.485), p = .003. This suggests that our results are also robust to a more stringent distinction criterion.

We also report Bayesian analyses (Rouder & Morey, 2012; Liang et al., 2008) for the result with the aggregate sample to provide further non-frequentist evidence of this effect. We employ a uniform model prior in our model comparisons. We only depict the null model and the best model (consisting of the intercept and the dummy for being a science communicator), though the model specification includes all control variables. We find a very high Bayes Factor at $BF_{10} = 202.98$ for the model consisting only of the dummy for being a science communicator (as well as the intercept). In our posterior coefficient summary, which relies on a JZS parameter prior with an r-scale of 0.354, we report the model-averaged (Hinne et al., 2020; Maier et al., 2022) coefficient as well as 95% credible intervals with the coefficient at −1.57 [−2.41, −0.76]. The results indicate further that this effect is robust even in a Bayesian framework, see Table 6 below.

Null Hypothesis 2 investigates whether, within scientific disciplines, perceiving one's work as more applied or theoretical is related to one's views on scientific realism. We used interval data as our dependent variable, ranging from '-3 Highly Theoretical' to '3 Highly Applied'. We also provide a robustness check that tests this with a binary dependent variable, in which responses on the applied side (1 through 3) were coded as '1', and all other values as '0', where we find the same set of results, see Table 7. The dependent variable is again the SRF1, but we also report the results also for the Full Aggregate Score in Appendix B with no difference in results.

We fail to find evidence that how applied or theoretical one perceives one's academic work predicts one's inclination towards realism. This is true for all academic disciplines studied, and the aggregate. To provide evidence in favour of a potential null, we conduct a series of equivalence tests for the regression coefficients from Table 7 with the interval variable against a series of plausible upper and lower equivalence bounds, reported in full in the appendix. Given our adjusted alpha at 0.0167, we only find evidence in favour of a null effect at [−1.5, 1.5] for the chemistry and biology samples, which does not allow us to estimate a tight null, but we do find strong evidence at [−0.5, 0.5] bounds for the aggregate sample, suggesting that its effect is close to zero, see Appendix A.

Lastly, for Null Hypothesis 3, we run two sets of regressions with the same specification as those for Null Hypothesis 1, except, as pre-registered, we use a single-item measure for selective realism and for voluntarism, see Table 8 for the results of the regression model testing selective realism.

We fail to find a difference between academics and science communicators in their agreement with selective realism. As before, we report equivalence tests in the appendix. We find that at the equivalence bound of [−0.75, 0.75] we have strong evidence for a null, though these bounds are quite wide (given the dependent variable is in the interval [1, 5]) and as such again do not allow us to conclude a tight null effect, see Appendix A.

With respect to voluntarism, we find that for chemists, as well as for the total sample, that science communicators score lower than academics. This effect is in the same direction as the effect we found on the full scientific realism scale, see Table 9. This is in accordance with our directional predictions. Also note that both science communicators, $t(136) = 4.71$, $p < .001$, $d = 0.402$, and scientists $t(129) = 8.86$, $p < .001$, $d = 0.777$ show mean responses that differ significantly from the neutral mid-point. We also conducted an exploratory analysis with respect to semantic realism, where we do not find any significant difference in the attitudes about semantic realism between scientists and science communicators, p = .234.

5. Discussion

In the discussion section, we want to focus on our pre-registered hypotheses first and then raise some additional points.

For Null Hypothesis 1, we predicted that science communicators would be more realist than their respective scientist counterparts. We find the opposite effect for some disciplines (such as biology and physics) and the aggregate sample, a data pattern that is also replicated on Bayesian analyses. This finding is contra to plausible theoretical predictions. One explanation might be that anti-realist attitudes are associated with a reflective attitude of not taking the findings of science at face value. Since science communicators occupy a position that necessities significant reflection on science, this might move them comparatively to the anti-realist side. Another explanation might be the patterns of semantic anti-realist communication. As we have discussed (section 1.4), communicators with lower scores on standard scientific realism (as indicated by SRF1) and who also score low on semantic realism can consistently produce realist communication patterns. If communicators lean more towards semantic anti-realism than scientists do, this could potentially explain why they generally display more anti-realist tendencies while still using realist surface language in their communications. Nonetheless, given our data, this explanation seems unlikely, as we observed no significant difference in the attitudes on semantic realism between scientists and science communicators.

In any case, this finding motivates further empirical and philosophical research. Note that the above finding is a comparative result, with science communicators being less realist than scientists, though both remain significantly more inclined towards realism.

Second, with respect to Null Hypothesis 2, we predicted that scientists working in applied fields would be more realist. Here, we fail to find

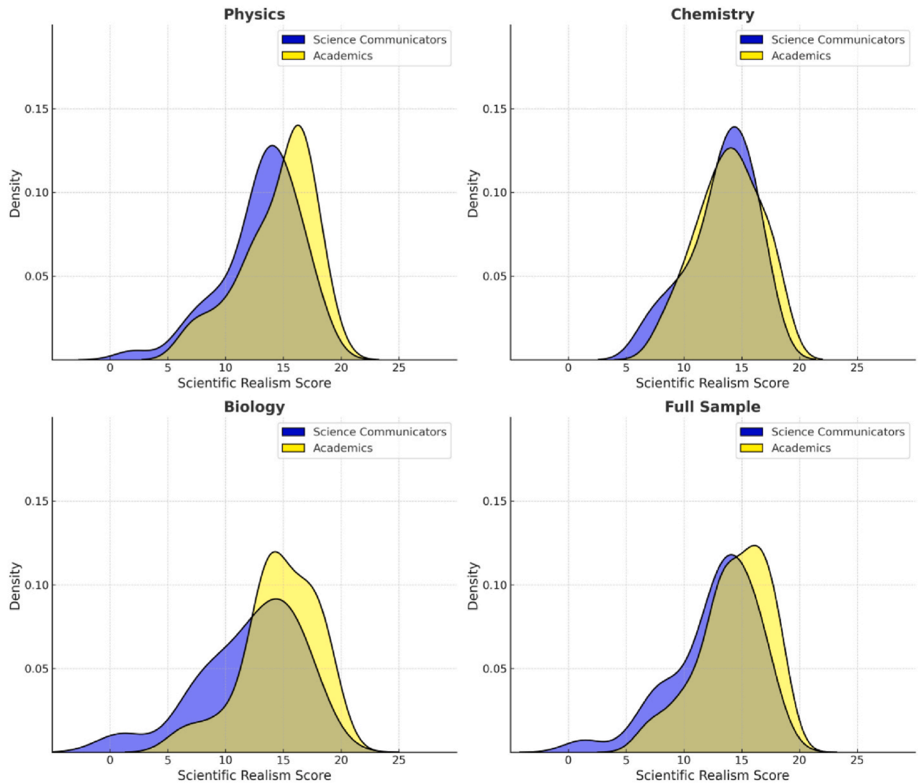

Fig. 5. Kernel density plot for group differences In realist views between academics and science communicators for the scientific realism scale.

Table 6
Bayesian model Comparisons and Posterior Coefficient Summary.

Model Comparison	P (M)	P(M| data)	BF_M	BF_{10}	R^2
Null Model	0.03	−0.000	0.05	1.00	0.00
Intercept + Science Communicator	0.03	0.33	14.98	202.98	0.06
Posterior Coefficient Summary	**Mean (SD)**	**Lower CI**	**Upper CI**	$BF_{Inclusion}$	
Science Communicator	−1.57 (0.43)	−2.41	−0.76	210.49	

Notes: Bayesian analysis for the aggregate results of Table 5.

Table 7
Regression predicting impact of perceived applied/theoretical work.

	Physics	Chemistry	Biology	Full Sample
Scientific Realism Scale (SRF1)				
Applied Work (Interval)	−0.030 (0.265)	−0.155 (0.384)	−0.482 (0.439)	−0.071 (0.177)
Control Variables	Yes	Yes	Yes	Yes
Adj. R^2	−0.077	−0.045	0.136	−0.006
n	56	37	37	130
Scientific Realism Scale (SRF1)				
Applied Work (Dummy)	−0.804 (0.877)	−1.336 (1.207)	−1.423 (1.216)	−0.607 (0.567)
Control Variables	Yes	Yes	Yes	Yes
Adj. R^2	−0.063	−0.009	0.139	0.006
n	56	37	37	130

Notes: OLS regressions predicting scores on the Full Scientific Realism Scale as well as the first extracted factor. Coefficients and standard errors in parentheses. *p < 0.1, **p < 0.05, ***p < 0.0167, ****p < 0.001.

statistically significant results but can estimate a tight null effect with equivalence tests at least regarding the aggregate sample. As such, we have evidence that there is indeed no effect (or only a negligibly small effect). Note that entity realists typically stress the tight connection between experimentalists and realist attitudes. Something similar might be said about scientists working more applied. They are closer to the tangible implications of their work, which should, from the entity realist perspective, in principle, motivate a stronger commitment to realism. Entity realism makes it plausible that there is a connection between 'doing' (whether experimenting or applying) and realist attitudes. Since our data shows that there is no such effect (or a negligibly small one), one might argue that this puts entity realists into trouble. There is, however, an explanation that is consistent with our data and entity realism. Since our scale mostly uses formulations of scientific realism in terms of theories, applied scientists might simply be more realist about *entities* than theorists but not more realist about *theories*. This is something Hacking (1983, pp. 263–265) explicitly mentions as a possibility. It might be interesting to test this possibility in follow-up work. In such a work, one might also want to investigate the relation between applied/pure and experimentalist/theoretical and thus contextualise our findings.[11]

Third, relating to Null Hypothesis 3, we expected science communicators to be more selectively realist and less voluntarist than their

[11] It is worth mentioning that one could attempt to indirectly assess this using our data, by hypothesizing that working as an applied scientist has a positive correlation with MR1 and PR, and a negative correlation with ER1 and NMA. We did not find any such statistically significant correlations. But given that any conclusion drawn from this would be hypothesizing after the results are known, we still rely on further data for testing this claim.

Table 8
Regression predicting difference between academics and science communicators regarding selective realism.

	Physics	Chemistry	Biology	Full Sample
Science Communicator	0.237 (0.236)	0.144 (0.262)	0.015 (0.232)	0.279** (0.136)
Control Variables	Yes	Yes	Yes	Yes
Adj. R^2	0.049	−0.038	0.089	0.045
n	104	65	88	267

Notes: OLS regressions predicting scores on the selective realism item. Coefficients and standard errors in parentheses.
*p < .1, **p < .05, ***p < .0167, ****p < .001.

Table 9
Regression predicting difference between academics and science communicators regarding voluntarism.

	Physics	Chemistry	Biology	Full Sample
Science Communicator	−0.031 (0.206)	−0.633*** (0.207)	−0.346* (0.188)	−0.294*** (0.110)
Control Variables	Yes	Yes	Yes	Yes
Adj. R^2	0.006	0.117	0.063	0.046
n	104	65	88	267

Notes: OLS regressions predicting scores on the voluntarism item. Coefficients and standard errors in parentheses.
*p < .1.
**p < .05.
***p < .0167.
****p < .001.

respective academic counterparts. For selective realism, we fail to find a significant difference. However, we did find the directionally predicted effect with respect to voluntarism. Science communicators in chemistry and in the aggregate showed lower scores than the corresponding academics, i.e., are less voluntarist.

We now also want to relate our results to the study done by Beebe & Dellsén. Many aspects of their study were replicated in our findings. For example, the prevalent inclination of scientists toward scientific realism. We only want to mention a few of the more surprising ones. One perhaps surprising fact, reported by Beebe and Dellsén (2020, p. 359), is that the van Fraassen items did not load onto the same factors as the rest of the standard scientific realism items, and they also did not load onto the same factor either. Even though we reformulated the second van Fraassen item (i.e., *item 6*) – it is now spelled out in terms of aims – we found the same effect. Furthermore, using said van Fraassen item as its own factor of indicating realism/anti-realism instead of our SRF1 we find that scientists are significantly more anti-realist. Beebe & Dellsén found the same effect and we now also replicated this for science communicators. As such, we replicated these two result patterns despite our reformulation, giving additional credence to the robustness of these effects. As Beebe and Dellsén (2020, p. 359) argue, this finding indicates that van Fraassen did change the central questions of scientific realism significantly from its classical formulations (Beebe & Dellsén, 2020, pp. 359–361).

Furthermore, this result is also independently theoretically plausible for science communicators of the two theorized types – (i) semantic realists and (ii) semantic anti-realists (see section 1.4). Concerning (i), on the one hand, they can simply be straightforwardly realist on vFR1 and vFR2. On the other hand, for vFR1, communicators might contend that scientists need not be realists to achieve practical success, even if they themselves embrace realism. For vFr2, they might hold that the primary goal of science is empirical adequacy, while concurrently identifying as realists of type (i). Concerning type (ii) and vFr1, they might simply choose the anti-realist option, or they might see realism as practically advantageous. Indeed, Feyerabend espoused this view, opining that anti-realism could sap scientists' motivation (see Van Fraassen, 1980: 93). Concerning type (ii) and vFr2, again semantic anti-realists might diverge about what they say about the ultimate aim of science. As such, since also from a philosophical standpoint, it is reasonable for science communicators to perceive the van Fraassen items to be independent of classical realism, it also makes sense that the van Fraassen items were once again found to be independent of the traditional understanding of scientific realism (as depicted by SRF1) in our study.

The only central difference between our observed factor structure and Beebe & Dellsén's is that we did not find Epistemic Realism to load onto our central SRF1. Beebe and Dellsén (2020) did not test Semantic Realism, which did not load onto our main factor either. This is interesting, because it shows that scientists and science communicators perceive the question of semantic realism separate from the realism cluster – a perception they share with many philosophers of science in the realism debate nowadays. As expected, and similar to Beebe & Dellsén's findings, the no-miracle argument item, expressing the central argument for realism, loaded on our SRF1. Concerning the Pessimistic Induction item (PI), Beebe & Dellsén surprisingly did not find any strong correlation between PI and their factors for (anti-)realism. We also found that the PI item loaded negatively on our SRF1. That means that participants who score higher on our SRF1 also tended to endorse PI more strongly. This outcome is puzzling and warrants further investigation. What can be said is that science communicators and scientists do not perceive the link between PI and standard forms of realism the way it is theorized by many philosophers of science.

Lastly, we want to discuss the significance of our results for the philosophical investigation of science communication. We indicated that there is some case-based evidence suggesting that current science communication leans towards realist expressions. Our study shows that science communicators indeed self-report more realist attitudes. This might partly explain realist science communication. On the other hand, the data shows that science communicators are less realist than scientists themselves. This was surprising, given circumstantial evidence of realist communication. It indicates at least that science communicators do not simply straight up copy the attitudes of scientists.

We indicated earlier, if we follow Cartwright's argument with respect to funding, that any overrepresentation of realist science communication might lead to wrong funding decision. Whether such an overrepresentation actually exists is apt to further empirical investigation. In any case, since our study showed that science communicators self-report more realist attitudes, one way of combating any disbalance we might find, might be to educate science communicators better on the anti-realist perspective. On the other hand, science communicators also showed significantly less voluntarist attitudes than scientists. As such, there might be some resistance to communicate a more nuanced picture since scoring lower on the voluntarist item indicates a higher inclination to think that only one side in the realism debate is in fact correct. Still, since science communicators already self-report less scientific realist attitudes than scientists themselves, if there is indeed an overrepresentation of realist communication as the circumstantial evidence suggest, then this might not even have a strong explanation in their realist attitudes. For topics that demand a more nuanced communication, science communicators may simply need to be equipped with better tools to communicate the anti-realist standpoint or they simply need to be more aware of how to communicate the anti-realist side.

Lastly, it is crucial to consider how the general public deciphers the messages conveyed by science communicators. While the interplay between scientific realism and public perception has not been thoroughly explored yet, there is a wealth of research and theorizing about how the public interprets scientific discourse. A notable example is Kovaka's (2019) study on climate science denial. Kovaka posits that a segment of the population, despite supporting scientific endeavors, denies climate science. Their skepticism stems from misunderstandings about the provisional nature of scientific knowledge, the methodologies used, and the principles of objectivity. Consequently, they perceive climate science as failing to uphold the rigors of scientific standards, leading to increased skepticism towards climate-related communications. A similar mismatch explanation is argued for in Gerken (2022).

Transferred to science reporting in the context of the realism debate, it is plausible that pre-existing beliefs will influence the reception here as well. For instance, if someone leans towards anti-realist views in certain scientific domains, they might approach realist-centric science communications with skepticism, and vice versa. This underscores the point that even if science communicators perfectly mirror the current state of the scientific realism debate, it does not guarantee that the general audience will interpret it as intended.

As such, our investigation into the attitudes of science communicators regarding scientific realism, while providing significant insights, has its inherent limitations when translated directly to the public's understanding – the straightforward translation of realist and anti-realist attitudes in science communication does not guarantee a clear and accurate reception by the public. However, this realization does not diminish the value of our research. Rather, it prompts us to consider the factors that influence public perception, adding depth to our study's implications. Understanding the nuances in the attitudes of science communicators is the first step. The next would be to explore how these attitudes interact with the preconceptions of the public to shape their understanding of science and its broader philosophical underpinnings. Future research should delve into the ways in which scientific realism is received by various segments of the public. Surveys, focus groups, and in-depth interviews can provide insights into the pre-existing beliefs of individuals and how these interact with the information presented to them. Such studies would be instrumental in bridging the gap between science communication and public interpretation.

In conclusion, our research sets the stage for a comprehensive exploration of the dynamics between scientific realism in science communication and public understanding. By shedding light on the attitudes of science communicators, we hope to stimulate further inquiry into the broader ecosystem of science communication and its multifaceted impact on society.

6. Limitations

One main limitation of the present study is its low response rate. Low response rates by themselves are not scientifically problematic and quite common in contexts of cold emailing experts (Houkoop et al., 2018). After all, to hit acceptable sample sizes a low response rate can always be compensated for with an increase in the denominator. What makes low response rates problematic is that they may lead to a nonresponse bias (Cull et al., 2005; Groves, 2006), meaning that participants who respond may be fundamentally different from those who do not. We acknowledge that this may pose a challenge for our study, but we provide the following evidence that our study is at least not heavily affected by nonresponse bias. The way we do this is by looking at the difference in demographics between the email list population and our sample. Because we do not have access to the gender distribution of all those whose emails we collected, we drew on the fact that we split collected email addresses into faculty and graduate students. Specifically, roughly 54% of emails we sent to the academic sample were directed at graduate students. In our data, we find that 49% of respondents reported not having finished a PhD, a mere 5 percentage point difference. This means that while our sample at hand does not perfectly match the population we drew on, there is some evidence that while our response rate is low, our response representativeness is, at least along some dimensions, reasonable.

However, low response rates coupled with unrepresentative sampling as may have occurred in our study due to relatively lax inclusion criteria may interact in a way that reduces the generalisability of our results. This is because selection bias may lead to an unrepresentative sample which, coupled with low response rates, may make inferences to the underlying population of interest more difficult. And while we were able to provide robustness checks that ensure that our science communicator sample did not include scientists, we were not able to do the reverse for the scientist sample, so some representativeness issues may remain with respect to the scientist sample. We acknowledge this difficulty in the data we present and wanted to flag this for readers.

A second limitation is that the sample sizes are moderate to small. While we did not pre-register any a priori power analysis because we knew that our maximum sample size was constrained by factors outside our control (i.e. response rates), the smaller than expected sample size may have contributed to some of the results where we fail to find a significant effect all the while not providing a tightly estimated null via equivalence tests. While we do explicitly outline this weakness in the main text, we believe it is important to outline it here again.

Overall, these considerations substantially limit the generalisability of our results. This suggests that wider conclusions about the populations might have to be accompanied by clear statements of the methodological trade-offs and limitations that low-sample studies with less-than-optimal selection criteria have. However, because the population is difficult to sample from and because we stuck to a pre-registered analyses plan, we argue that our results still provide novel data that may help us understand the differences in views between scientists and science communicators.

Data availability

Data is available on the OSF file: https://osf.io/nc7qy/.

Appendix A

Here, we provide direct evidence in favour of the null, which is something that a standard null-hypothesis testing framework technically cannot provide. Specifically, we present equivalence test results (Lakens et al., 2020). These amount to two one-sided t-tests (TOST) against two equivalence bounds that allow for the conclusion that the estimate is null or negligibly small, i.e., within these bounds. Following Alter and Counsell (2021), we conduct these tests on the regression coefficients from our main analyses and test them against a range of plausible upper and lower equivalence bounds. We focus on the appliedness variable on the interval scale from Table 7 with the Scientific Realism Scale (Factor 1) as the dependent variable. We only find evidence for the null at our adjusted alpha level at equivalence bounds of [−1.5, 1.5] for the chemistry and biology samples, meaning that we cannot estimate a tight null effect. For physicists, the equivalence bounds are significantly tighter at [−1.0, 1.0], while for the aggregate sample, we can estimate a relatively tight null effect at the [−0.5, 0.5] bounds.

Appendix Table 1
Tests of Equivalence for Regression Coefficients Predicting Scientific Realism

	−0.5	0.5	−1.0	1.0	−1.5	1.5
Sci. Realism Scale (F1)						

(continued on next page)

Appendix Table 1 (*continued*)

	−0.5	0.5	−1.0	1.0	−1.5	1.5
Physics	1.77**	2**	3.66****	3.88****	5.55****	5.77****
Chemistry	0.90	1.71**	2.20**	3.01***	3.50****	4.31****
Biology	0.04	2.24**	1.18	3.38****	2.32**	4.51****
Full Sample	2.42***	3.23****	5.25****	6.05****	8.07****	8.88****

Notes: All *t*-test results for TOST procedures on a variety of lower and upper equivalence bounds (in unstandardized coefficients). *p < 0.1, **p < 0.05, ***p < 0.0167, ****p < 0.001.

We also report a set of equivalence tests for our results from Table 8 for our models predicting selective realism. As above, we only find evidence for a null at the very wide equivalence bounds of [−0.75, 0.75], such that we cannot state with high confidence what the relationship between selective realism and science communication work is.

Appendix Table 2
Tests of Equivalence for Regression Coefficients Predicting Selective Realism

	−0.25	0.25	−0.5	0.5	−0.75	0.75
Physics	2.06**	0.06	3.12***	1.11	4.17****	2.17***
Chemistry	1.50*	0.40	2.43***	1.36*	3.41****	2.31***
Biology	1.14	1.01	2.22***	2.09**	3.29****	3.17***
Full Sample	3.91****	−0.22	5.75****	1.63*	7.60****	3.48****

Notes: All *t*-test results for TOST procedures on a variety of lower and upper equivalence bounds (in unstandardized coefficients).
p < 0.1, **p < 0.05, ***p < 0.0167, ****p < 0.001.

Appendix B

We report a further robustness check for Table 7, where we report the same models that have the applied/theoretical variable both as an interval and as a dichotomised dummy. As pre-registered, we use the Full Aggregate Score as our dependent variable. We also fail to find statistically significant effects for all subgroups and the aggregate, indicating the same results as those reported in the main text, lending additional robustness to the results.

Appendix Table 3
Regression Predicting Impact of Perceived Applied/Theoretical Work

	Physics	Chemistry	Biology	Full Sample
Full Aggregate Score				
Applied Work (Interval)	−0.246 (0.266)	−0.130 (0.386)	0.464 (0.430)	−0.096 (0.178)
Control Variables	Yes	Yes	Yes	Yes
Adj. R^2	−0.028	−0.069	0.167	0.011
n	56	37	37	130
Full Aggregate Score				
Applied Work (Dummy)	−0.952 (0.878)	−0.023 (1.240)	0.788 (1.242)	−0.222 (0.575)
Control Variables	Yes	Yes	Yes	Yes
Adj. R^2	−0.021	−0.073	0.141	0.010
n	56	37	37	130

Notes: OLS regressions predicting scores on the Full Scientific Realism Scale as well as the first extracted factor. Coefficients and standard errors in parentheses.
*p < 0.1, **p < 0.05, ***p < 0.0167, ****p < 0.001.

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